Dynamics and Hydrostatics. By R. H. Pinkerton, B.A. (London: Blackie and Son, 1888.)

THIS is a first course of dynamics intended for the use of science classes and colleges, and specially adapted to the requirements of the Science and Art Examinations in theoretical mechanics. The subject is treated mathematically, but the mathematical knowledge required for an intelligent perusal of the book is limited to elementary algebra and trigonometry. The fundamental units are thoroughly well explained, and, which is saying a great deal, they are used consistently throughout. Every important proposition is followed by a number of good examples fully worked out, and many others are given as exercises.

The book is excellently adapted to the Second Stage of the Science and Art Syllabus, and teachers will not have much difficulty in selecting the portions suitable for students working for the First Stage. It is also well adapted for the use of students working at the subject for the London Matriculation and other University Examinations. But, notwithstanding these qualifications, it is thoroughly conscientious. In fact, from a mathematical point of view, the book leaves nothing to be desired, but in this practical generation a greater number of illustrations from every-day life would not have been out of place. A. F.

Geography for Schools. By Alfred Hughes, M.A. Part I. Practical Geography. (Oxford: At the Clarendon Press, 1887.)

THERE are many signs that the study of geography will in future take a much more important place in the ordinary school course than has hitherto been assigned to it. Even from the point of view of those severely practical persons who care little about the purely intellectual aspects of education, there can be no doubt as to the value of the kind of geographical knowledge with which this book is chiefly concerned; and the subject, if properly treated, is one in which young scholars may easily be led to take genuine interest. The present volume will be of great service to schoolmasters who may wish to make a fresh start in geographical teaching. It is based, as Mr. Hughes explains, on the results of seven years' experience in the modern side at the Manchester Grammar School; and no one who examines the book will be surprised that he has found it possible, within the limits of an ordinary term's geographical course, to give instruction on many classes of problems which are not usually treated at school. He begins with the consideration of latitude and longitude, and with rules for the drawing of maps from the atlas and from memory. He then deals with the measurement of the distance between two places on the earth's surface, and explains the rotation of the earth, with the consequent difference in the time of day at two places on the earth. The remaining subjects are the apparent movements of the fixed stars; the Pole star; Polar distance; the apparent movements of the sun; the seasons; meridian altitude of the sun; declination; the length of day and night at any time and place; the sun's altitude; place of sunrise and sunset; the length of twilight; apparent and Greenwich mean time; movements of the earth; the length of shadows; the distance to be seen from mountain summits; the trade winds; and the calendar. The questions connected with these subjects are discussed in a way that secures the combination of geography, geometrical drawing, arithmetic, and the elementary ideas of geometry; and the author's aim is to induce the student to think for himself, rather than to burden his memory with disconnected facts. It is hardly necessary to say how much better this is than the learning of the names of capes, mountains, rivers, &c., by heart. With such a work in their hands, teachers should be able to make lessons in geography a most useful introduction to the study of some important branches of scientific method.

Key to Todhunter's Differential Calculus. By H. St. J. Hunter, M.A. (London: Macmillan and Co., 1888.)

THIS "Key" will be extremely useful to those who are teaching the subject, but more so to those who are getting it up by themselves. The examples are worked out in a clear and intelligible manner, the geometrical problems being so worded that the student can supply figures to enable him more readily to follow the reasoning. To the chapters on "Curve Tracing" and "Miscellaneous Propositions" the author has added figures; and in the solutions to some of the examples in chaps. xi., xiii., xv., xx., and xxii., improved methods have been adopted, making the book more useful and complete. Great care seems to have been taken to insure accuracy.

Electrical Instrument Making for Amateurs. By S. R. Bottone. (London: Whittaker and Co., 1888.)

IN this little book the author has placed before the reader very good and economical methods of making the more useful pieces of electrical apparatus, using only tools of the simplest kind, such as may be found in any household. The instructions are given in a clear and simple manner, and are illustrated by woodcuts, showing the various parts of the apparatus, with the proportions marked on them. Those who are attending courses of lectures on this subject will find this volume immensely useful, as a more thorough and practical insight is obtained by making and using these instruments, however rough, than by mere reading.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Language = Reason.

PROF. ST. GEORGE MIVART has read my letter on "Language = Reason" in NATURE of February 2 (p. 323) with very great care, and I feel grateful to him for several suggestive remarks. But has he read the heavy volume to which that letter refers—my "Science of Thought"? I doubt it, and have of course no right to expect it, for I know but too well myself how difficult it is for a man who writes books to read any but the most necessary books. I only mention it as an excuse for what might otherwise seem conceited—namely, my answering most of his questions and criticisms by references to my own book.

Prof. Mivart begins by asking why I should have explained reasoning by reckoning.

Now, first of all, from an historical point of view—and this to a man who considers evolution far more firmly established in language than in any other realm of Nature is always the most important—the Latin *ratio*, from which came *raison* and our own *reason*, meant originally reckoning, casting up, calculation, computation, long before it came to mean the so-called faculty of the mind which forms the basis of computation and calculation, judgment, understanding and reason.

Secondly, I began my book on the "Science of Thought" with a quotation from Hobbes, that all our thinking consisted in addition and subtraction, and I claimed the liberty to use the word thinking throughout my own book in the sense of combining. Such a definition of thinking may be right or wrong, but provided a word is always used in the sense in which from the beginning it has been defined there can at all events be no misapprehension nor just cause of complaint on the part of the critic

What I meant by combination, or by addition and subtraction being the true character of thinking, I explained very fully. "Any book on logic," I said, "will teach that all our propositions are either *affirmative* or *negative*, and that in acquiring or communicating knowledge we can do no more than to say that A is B, or A is not B. Now, in saying A is B, we simply add A to the sum already comprehended under B, and in saying A is not B, we subtract A from the sum that can be comprehended under B. And why should it be considered as lowering our high status, if what we call thinking turns out to be no more than adding or subtracting? Mathematics in the end consist of nothing but addition and subtraction, and think of the wonderful achievements of a Newton or a Gauss—achievements before which ordinary mortals like myself stand simply aghast."

Prof. Mivart holds that there are but two forms of intellectual activity: (1) acts of intuition, by which we directly apprehend certain truths, such as, *e.g.*, our own activity, or that A is A; and (2) acts of inference, by which we indirectly apprehend others, with the aid of the idea "therefore."

There is a wide difference between our apprehending our own activity and our apprehending that A is A. Apprehending our own activity is inevitable, apprehending that A is A is voluntary. Besides, the "therefore" on which Prof. Mivart insists as a distinguishing feature between the two forms of thought is present in the simplest acts of cognition. In order to think and to say "This is an orange," I must implicitly think and say, "This is round, and yellow, has a peculiar skin, a sweet juice," &c.; *therefore* it is an orange. The "therefore" represents in fact the justification of our act of addition. We have by slow and repeated addition formed the concept-name orange, and by saying "This is an orange," we say no more than that we feel justified, till the contrary is proved, in adding this object before us to the sum of oranges already known to us. If the contrary is proved, we subtract, and we add our present object either to the class and name of lemons, citrons, &c., or to a more general class, such as apples, fruit, round objects, &c. We ought really to distinguish, as I have tried to show, not only two, but four phases in every act of cognition, viz. sensation, perception, conception, and naming; and I contend that these four phases, though distinguishable, are not separable, and that no act of cognition is perfect without the last phase of naming.

But how is it, Prof. Mivart continues, that different words in our language have one meaning, and different meanings one word? Does not this show that thought and language cannot be identical?

It has been the principal object of all my mythological studies to account not only for the origin of *polyonymy* and *homonymy*, but to discover in them the cause of much that has to be called mythology, whether in ancient tradition, religion, philosophy, or even in modern science. I must therefore refer Prof. Mivart to my earlier writings, and can only mention here a few well-known cases of mythology arising from polyonymy and homonymy.

We can easily understand why people should have called the planet Venus both the morning and the evening star; but we know that in consequence of these two names many people have believed in two stars instead of one. The same mountain in Switzerland is called by the people on the south side *Blackhorn*, by the people on the north side *Whitehorn*, and many a traveller has been misled when asking his way to the one or the other. Because in German there are two words *Verstand* and *Vernunft*, originally meaning exactly the same thing, German metaphysicians have changed them into two distinct faculties, and English philosophers have tried to introduce the same distinction between the understanding as the lower and reason as the higher faculty.

Nothing is really easier to understand, if only we consult the ancient annals of language, than why the same object should have had several names, and why several objects should have had the same name. But this proves by no means that therefore the name is one thing and the concept another. We can distinguish name and concept as we distinguish between the concave and convex sides of a lens, but we cannot separate them, and in that sense we may call them inseparable, and, in one sense, identical.

Lastly, Prof. Mivart starts the same objection to my system of psychological analysis which was raised some time ago in these columns with so much learning and eloquence by Mr. Francis Galton. He appeals to his own experience, and maintains that certain intellectual processes take place without language. This is generally supposed to put an end to any further argument, and we are even told that it is a mistake to imagine that all men are alike, so far as their psychological processes are concerned, and that psychologists should study the peculiarities of individuals rather than the general character of the human intellect. Now, it seems to me that *l'un n'empêche pas l'autre*, but that in the end the object of all scientific inquiry is the general, and not the individual. The true life of language is in the dialects, yet the grammarian aims at a general grammar. In the same way the psychologist may pay any amount of attention to mere individual peculiarities and idiosyncrasies; only he ought never to forget that in the end man is man.

But it does not even seem to me that intellectual processes without language, as described by Mr. Galton and Prof. Mivart, are at all peculiar and exceptional. I have described similar cases, and tried to account for them, in different parts of my book. If Prof. Mivart says that "a slight movement of a finger may give expression to a meaning which could only be thought in words by a much slower process," I went much further by saying that "silence might be more eloquent than words."

Mr. Galton asked me to read a book by Alfred Binet, *La Psychologie du Raisonnement*, as showing by experiments how many intellectual acts could take place without language. I read the book with deep interest, but great was my surprise when I found that M. Binet's observations confirmed in the very strongest way my own position. I had shown how percepts—that is, images—could exist with a mere shadow of language, and that nothing was more wonderful than what Leibniz called the algebra of thought. Now, what do M. Binet's experiments prove? That there are two kinds of images, the *consecutive*, reproduced spontaneously and suddenly, and the *memorial*, connected with an association of ideas. The *consecutive* image, a kind of impression *avant la lettre*, may reappear long after the existing sensation has ceased to act, and it reappears without any rhyme or reason. But how are the memorial images recalled, seen by people, such as M. Binet describes, in a state of hypnotism? Entirely by the word. Show a hypnotized patient her portrait, and she may or may not recognize it. But tell her, in so many words, "This is your portrait," and she will see her likeness in a landscape of the Pyrenees (pp. 56-57). M. Binet is fully aware of what is implied by this. Thus, on p. 58, he writes: "*L'hallucination hypnotique est formée d'un image suggérée par* la parole." So, again, when describing the simplest acts of perception, M. Binet explains how much is added by ourselves to the mere impressions received through the senses by "*ce qu'on croit voir*," by "*ce qu'on croit sentir*," and by "*le nom qu'on croit entendre prononcer.*" The facts and experiments, therefore, contained in M. Binet's charming volume seem to me entirely on my side, nor do I see that that thoughtful observer has ever denied the necessity of language or signs of some sort for the purpose of reasoning, nay even of imagination.

I find it difficult to answer all the questions which the Professor has asked, because it would seem like writing my own book over again. However, I shall confess that I have laid myself open to some just criticism in not renouncing altogether the metaphorical poetry of language. I ought not to have spoken of Truth as a kind of personal being, nor of Reason as a power that governs the universe. But no astronomer is blamed when he uses the old terminology of sunrise and sunset; no biologist is misunderstood when he speaks of mankind; and no philosopher is denounced when he continues to use the big I instead of "succession of states of consciousness." If, therefore, I said that I recognized in evolution the triumph of reason, I meant no more than that I could not recognize in it the triumph of mere chance. Prof. Mivart imagines that I misunderstood what the biologist means by the survival of the fittest. Far from it, I understand that phrase, and decidedly reject it. For, either the survival of the fittest means no more than that that survives which is able to survive,—this would be mere truism and a patent tautology,—or, if we take in the whole circumstance of Nature, the survival of the fittest implies some kind of inherent fitness and reasonableness. Prof. Mivart writes: "What there is less reasonable and right in a Rhytina than in a Dugong, or in a Dinornis than in an Apteryx, would, I think, puzzle most of our zoologists to determine; nor is it easy to see a triumph of reason in the extermination of the unique flora of St. Helena by the introduction of goats and rabbits." No doubt, it is not easy to see this. But need I remind Prof. Mivart that many things may be true, though it is not easy to see them? We often do what we think is reasonable and right, though we seem to see nothing but mischief to ourselves and others arising from our acts. Why do we do this? Because we believe in the ultimate triumph of reason and right, though it may take millions of years to prove that right is right. I have the same faith in Nature; and, taking my stand on this scientific faith, I believe that natural selection must in the end prove rational selection, and that what has vaguely been called the survival of the fittest will have to be interpreted in the end as the triumph of reason, not as the mere play of chance. F. MAX MÜLLER.

Oxford, February 21.

"Coral Formations."

CAPTAIN WHARTON's paper on coral formations in last week's NATURE (p. 393) will have been read with great interest by all who have examined and studied coral reefs. It is unlikely that any objections will be raised to the illustrations he has brought forward of how the coral plantations may be built up from deeply submerged banks, and eventually formed into complete atolls and barrier reefs at a great distance from continental and other shores. The mode of formation has been dwelt upon by Le Conte and Guppy in the case of barrier reefs, and I have pointed out the same thing in my remarks about the Maldive and similar atoll groups. The instances cited by Captain Wharton are of great value, especially as he has been able to consult large manuscript plans.

Captain Wharton apparently considers that the solution of carbonate of lime by sea-water plays no important part in deepening, widening, and modifying the form of such atolls and barrier reefs; in this I cannot agree with him.

By reference to what is now taking place in Nature, as well as to experiments conducted in the laboratory, it has been shown that the solution of the carbonate of lime of dead shells and skeletons by the sea is as constant and universal as its secretion by the living organisms. From some considerations which I recently laid before the Royal Society of Edinburgh, it is probable that there is more secretion and deposition of carbonate of lime in the ocean, as a whole, than removal by solution, and it is almost certain that at the present time there is a vast accumulation of carbonate of lime going on within the coral-reef regions of the ocean. The amount of secretion becomes less with increasing depth beyond one hundred fathoms, and laboratory experiments under great pressures have shown that the rate of solution becomes greater with increasing depth; but both processes are always in action wherever there are life and growth, death and decay. In some regions secretion is in excess, and there is a formation of calcareous deposits; in others solution is equal to secretion, as over the red clay areas of the ocean. Again, solution may be in excess of secretion, as in the larger and more perfect coral lagoons. The *rôle* of carbonate of lime in the ocean may not inaptly be compared to that of aqueous vapour in the atmosphere over land surfaces. Where precipitation is in excess of evaporation, fresh-water lakes are formed, and rivers carry the surplus water down to the ocean; where evaporation is in excess, there is a formation of inland drainage areas, deserts, and salt lakes.

In small coral atolls the periphery is large relatively to the size of the lagoon, and the secretion of lime and formation of coral sand are greatly in excess of the solution that takes place, hence the lagoon becomes filled up; in it are frequently found deposits of sulphate of lime, guano, magnesian and phosphatic rocks. On the other hand, when a comparatively large atoll reaches the surface, the periphery being small relatively to the size of the lagoon, there is less secretion and formation of coral sand by the living outer surface than is removed in solution from the lagoon; it is in consequence widened, deepened, and reduced to a more or less uniform appearance, while the islands on such reefs never, so far as I know, contain deposits of sulphate of lime, guano, magnesian or phosphatic rocks. On open banks, such as the Macclesfield and Tizard Banks, the coral sand is generally largely made up of bottom-living Foraminifera, Polyzoa, Serpulæ, and Calcareous Algæ, and the bank may be rising from the secretions of these organisms; but when the peripheral reefs reach the surface the conditions become more or less inimical to vigorous growth, and in a perfect atoll the fine calcareous mud is removed at a relatively rapid rate.

My answer to Captain Wharton's question is that in all normal conditions the extent of surface in the shell, coral, or fragment of coral sand exposed to the action of sea-water compared with the mass determines the rate at which these organisms will disappear in solution. It is improbable that this action is extremely slow at the bottom of the deep lagoons. Independently of the mixing by convection currents, even a very slight wind over the surface of the lagoon will set the whole water in motion. This is clearly shown by my observations in the western lochs of Scotland, which are much deeper than any lagoon; a moderate breeze produces motion at a depth of sixty fathoms in a very short space of time. The water mixed up with the mud at the bottom is thus changed long before the point of saturation is reached.

I have never seen any wide extent of fringing reef but what was very deeply cut up with channels, and from Captain Wharton's own description this appears to be the case at Rodriguez. That a ship channel has not there been formed is probably due to the shallow water surrounding the island and the probably rapid growth outward of the reef; the average depth outside the reef is usually less than ten fathoms, and at a distance of two miles seaward it is only from twenty to thirty fathoms. In some instances the large proportion of Calcareous Algæ on the reefs appears to compensate for the removal in solution, and thus to retard the formation of ship channels.

I doubt if any recent writer has attempted to give an "explanation which will fully account for the almost infinite variety of coral formations." It is unnecessary to state that each reef must have peculiarities depending on the nature and form of its foundation, and the meteorological and other conditions of the seas in which the reef is situated; it is only by a careful and detailed study of all these conditions that the peculiarities of any individual reef can be fully explained. At the same time it appears to me beyond doubt that the general and well-known characteristic features and form of coral reefs can be accounted for by reference to certain general considerations, chief among these being the vigorous growth of reef-forming species in positions and at depths where the supply of pelagic oceanic organisms, which form their food, are most abundant, and the removal of dead coral and coral debris wherever this is exposed to the action of sea-water.

Captain Wharton calls attention to our imperfect knowledge of the coral groups of the Pacific, but he understates the case in saying "that the waters of the Fiji and the Society Islands are the only ones which can be said to be in any sense surveyed." Cook, Kotzebue, Duperrey, Beechy, and Wilkes have given running surveys of many of the Paumotus, and we know something about the depths inside and outside of a good many of them. We know much about the islands containing guano. The French have made some excellent charts of the New Caledonia reefs, and the Americans have done the same for some of the Hawaiian Islands. Captain Wharton will acknowledge that we have a splendid survey of the Maldives, the most extensive group of atolls in the world; the islands marked with names in this British Survey number 602. Other groups in the Indian Ocean are well surveyed, and nearly all the Atlantic reefs have been correctly laid down on charts.

I feel sure that all who take an interest in this subject will hope for many more contributions from Captain Wharton's pen on coral formations. JOHN MURRAY.

I HAVE read with great interest the article on coral formations in your last number (p. 393), by Capt. Wharton. It is not because I wish to claim to have anticipitated the views which he gives as to the formation of atoll lagoons and barrier reef lagoons that I am writing to state that at the very date of the publication of Capt. Wharton's article I was engaged in writing a paper on coral formations, based upon a study of living corals at Diego Garcia, and on a consideration of the great submerged atolls known as the Great Chagos Bank and the Pitt and Centurion Banks, situated north and west of that island, in which I arrive at conclusions nearly identical with his. It has seemed to me, as it has to him, that the solution of dead coral rock in the interior of a reef does not sufficiently account for the formation of lagoons, and that the true cause of the atoll and barrier lagoons surrounded either by a reef which is awash, or by a strip of low land, lies in the peculiarly favourable conditions for coral growth present on the steep external slopes of the reef. In Diego Garcia I observed that although the shore reefs are for the most part covered with 1 or 2 feet of water, even at the lowest spring tides, yet their flat surfaces are nearly invariably barren of growing coral. Just at their edges, however, and on the steep external slopes beyond the edges, reef-building corals grow luxuriantly. According to Capt. Moresby, quoted by Mr. Darwin in his book on "Coral Reefs," the flat surface of the rim of the Great Chagos Bank is barren of living corals,